

Separate Disciplines: The Study of Behavior and the Study of the Psyche

Lawrence E. Fraley
West Virginia University

Ernest A. Vargas
West Virginia University

The study of behavior differs fundamentally from the study of the psyche and logically cannot share the same discipline. However, while disciplines might be *defined* through technical exercises, they *function* through exercises of political power. The evolution of a discipline, though based on field and laboratory data interpreted within a specific paradigm and justified publicly by its utility to solve personal and social problems, follows a course of development in the political arenas of the academies and the professions. We happen to have a discipline, roughly connoted by the label "behavior analysis," without an academic home (the present ones haphazardly tolerate our activities), without a professional organization (the present one lobbies only "for behavior analysis"), and without a true professional name (the present one implies an approach not a discipline). No scientific community lasts long without a supporting professional infrastructure. In explicitly asserting ourselves as a discipline, we confront a number of difficult issues such as continuing to work in departments antithetical to behaviorism and a number of problems such as what we call ourselves to identify our professional and scientific concerns. (For example, we need a term descriptive of our science in its broad sense. That term is not psychology. Too many people persist in maintaining its commitment to cognitivism. On whatever term we agree, "behavior" should constitute its stem, for our efforts focus there, not in the putative underlying psyche or its current cognitive update.) The focus of our concerns and the solutions of our problems rest on one issue: Will our discipline prosper most as a branch of psychology or as an independent discipline? Slowly, but surely, our actions demonstrate that the latter is the preferred option, but these actions, though fortuitous, occur almost by accident. By specifically programming to achieve an independent professional status we increase the probability of doing so.

B. F. Skinner writes books that do not seem much like psychology books and that certainly do not resemble psychology textbooks. Rather, they form a core of literature better described as a comprehensive basic science of human behavior. The books lay out a philosophy of science, called radical behaviorism, that provides the epistemological foundations for what he calls the experimental analysis of behavior. They describe voluminous data acquired through experimental work in the laboratory. And equaling the importance of the conceptual and experimental foundations, in these books Skinner also suggests many practical technologies for applying the basic principles in various domains of everyday concern. Skinner's writings do no more than sketch out these technologies. But based on the variety of applications suggested, Skinner clearly puts

himself among those who recognize "behavior analysis" as the comprehensive basic science necessary for effective work in any applied discipline dealing with human behavior.

"Behavior analysis" is not a profession. It remains in large part a scientific paradigm. To know that we are analysts of behavior is to know how we approach behavioral problems, but not what we do for a living. Building upon the common basic science of the experimental analysis of behavior, each of us, as a practitioner, whether in laboratory or classroom or clinic or factory or other settings, acquires and extends the particular behavioral technology apropos of each of our respective professions. We see individuals from different disciplines conduct experiments and discuss theory within the behavior analytic paradigm. And in the applications of behavior analysis, we see around us, for example, behavioristic educators, behavioristic sociologists, behavioristic clinicians, and behavioristic administrators. Concurrently, in all such disciplines and applied fields we encoun-

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ter practitioners who do not operate with the advantages provided by the science of the experimental analysis of behavior and the philosophy of radical behaviorism.

To prepare for work in a particular job that depends on managing or shaping behavior, a beginner must study the basic data and concepts of behavior analysis in the same way that a neophyte in the physical sciences studies the basic data and concepts of physics. The person's chosen profession then requires further study of a specific behavioral technology; for example, as an administrator, a teacher, or a clinician. The organized professional training programs in the behavioral disciplines necessarily teach their own behavioristic technologies. In addition, they often provide their own foundation curriculum in the basic science of "behavior analysis," as some practitioners currently call the link between the experimental analysis of behavior and the philosophy of radical behaviorism. Some specializations join with other closely related professions to offer common versions of the basic science. For example, training programs for teachers, school administrators, and specialists treating behavioral disorders and the behaviorally delayed might use in common a "scientific foundations" core in radical behavioral science, offered in the same college in which all of those training programs reside. Certain of the many behavior oriented training programs emphasize a small cluster of professional options that usually include some forms of clinical practice and perhaps some specializations in industry or education. These training arrangements often occur in psychology departments. They teach a few basics and dwell in detail on the behavioral technologies appropriate to those limited and specific professional options for which they have captured a monopoly in the educational marketplace.

One cannot argue that a clinician trained in a psychology department stands in greater need of the scientific framework of behavior analysis than a teacher trained in an education department or a

personnel manager trained in a business department. Psychologists, especially those working in psychology departments, therefore have no valid a priori right to claim ownership over radical behavioral science simply because they prepare persons for a few of the professional options based upon it. In fact, that issue is moot. Training programs in the basic science of behavior analysis operate outside of psychology departments. Clearly, as behaviorists, we can teach the scientific and philosophical foundations of behavior under the auspices of any department in any discipline, and do.

In establishing a discipline for all behaviorists, regardless of former professional affiliation, we confront several issues: (1) for behaviorists who happened to train as psychologists, the implications of continuing to work in departments of psychology, that is, departments devoted to a cognitive science, (2) what constitutes the background, range, and subject matter of behavior analysis, and (3) the problems in establishing behavior analysis as a separate discipline, those of academic home, professional organization, and professional name. Each issue discussed provides a necessary prologue for the succeeding one. The first issue pertains to the special problems of behaviorists working in the academic units that are supposed to turn out more behaviorists. The working environment for behaviorists in psychology departments usually leads to unhealthy outcomes both at a personal and discipline level, and gives little evidence of being anything but disadvantageous.

BEHAVIORISTS IN PSYCHOLOGY DEPARTMENTS

Behaviorists in psychology departments find themselves in a difficult position. This stems from a simple fact: Most psychologists are neither trained nor skilled in behavior analysis, nor even interested in it except to protest against it. Behavioral psychologists, especially radical behaviorists, find themselves in the minority both scientifically and politically. Psychology as a discipline reflects

strongly the surrounding culture and especially what the marketplace wants. The production units for the discipline, university academic departments, thus primarily produce clinicians and associated types who speak with the language of a philosophy of science in which intentions and feelings rule, not unlike the long-standing lay "common sense" position, but now hyped as cognitive science. Skinner (1984) gives some examples in his recent essay, "The Shame of American Education":

A short paper published in *Science* last April (Resnick, 1983) asserts that "recent findings in cognitive science suggest new approaches to teaching and mathematics" (p. 477), but the examples given, when expressed in noncognitive style, are simply these: a) Students learn about the world in "naive" ways before they study science; b) naive theories interfere with learning scientific theories; c) we should therefore teach science as early as possible; d) many problems are not solved exclusively with mathematics; qualitative experience is important; e) students learn more than isolated facts; they learn how facts are related to each other; and f) students relate what they are learning to what they already know. If these are *recent* findings, where has cognitive psychology been?

Cognitive psychology is frequently presented as a revolt against behaviorism, but it is not a revolt; it is a retreat. Everyday English is full of terms derived from ancient explanations of human behavior. We spoke that language when we were young. When we went out into the world and became psychologists, we learned to speak in other ways but made mistakes for which we were punished. But now we can relax. Cognitive psychology is Old Home Week. We are back among friends speaking the language we spoke when we were growing up. We can talk about love and will and ideas and memories and feelings and states of mind, and no one will ask us what we mean; no one will raise an eyebrow. (pp. 949-950)

Thus, professional psychologists in psychology departments and out, sort themselves out into two camps: the overwhelming majority who are developmentalists, Freudians, Rogerians, so-called humanists, information theorists, brain-mind epiphenomenalists, and so on—in short, cognitivists of all sorts—and, submerged among them, a small minority of behaviorists such as Kantorians and Hullians, and especially that constantly endangered few, the radical behaviorists. The ideological fervor of eclectic balance in the academic factory

requires that any minority view be suppressed in favor of an overview, which as a practical outcome, results in predominantly cognitive training. So virtually no pure behavioristic training program exists within any psychology department. In only a few institutions out of hundreds has the behavioral faction been able to attain a political majority and give its department a behavioral tone. Most of us can count those departments on the fingers of our hands, and depending on our inclinations to behavioral purity, not have to use both hands.

For every psychologist being trained behaviorally to assume a faculty position and shift the unfavorable ratio, far more are being trained to maintain that ratio. These individuals have only the vaguest acquaintance with radical behavior science and this reflects itself in the articles they write, and more seriously, in the textbooks they publish. Misconceptions, and even falsehoods, are far from few (Cooke, 1984; Todd & Morris, 1983). To argue that behavioristic psychologists should stay within psychology is, among other things, to promote a course that will maintain the political and professional minority status of behaviorists in psychology for a very long time. The nature and implications of that minority status must therefore be evaluated.

Many behavioral psychologists argue that they should remain within organized professional psychology—there to serve as agents of change. True, behavioral psychologists may induce change, since ineffective practitioners will eventually begin to copy the practices of more effective colleagues. But for behaviorists, neither group-acknowledgment nor leadership will necessarily follow. In professional disciplines, the majority, through action or avowal, prescribes both the knowledge base and the language of the discipline. Although minority practitioners might refine a principle, and the practices based upon it, both in their work and in the publications they control, the majority tends to ignore it until one or more of its members can find a way, through the work *they* do, to discover and refine the same principle in *their* work

and in *their* literature and speak of it in *their* terms. Only then will the majority give the work and its products professional recognition and wide dissemination.

A typical example appears in the newspaper of the American Psychological Association, the *APA Monitor*, under a headline announcing "Amos Tversky: His fundamental brilliance wins MacArthur prize" (Staff, 1984). Cognitive psychologist Tversky, whose work on belief and preference the article describes as work on "psychological quantities," is portrayed by social psychologist Lee Ross of Stanford University as "almost the perfect representative for psychology." Setting forth aspects of his own work that figured most heavily in his receiving the award, Tversky discussed a major finding during his work on judgement:

Contrary to normative axioms, people's preferences depend greatly on the way decision problems are framed. So different ways of framing the same decision problem could lead to drastically different preferences. (p. 3)

In *Science and Human Behavior*, over thirty years ago in a chapter entitled "Thinking," Skinner revealed how different ways "of framing the same decision problem" could lead to different preferences:

For more direct results [in making a decision] we resort to the manipulation of stimuli. If all relevant courses of action show some strength before we decide among them, our techniques consist of finding *supplementary* sources of strength which, when applied to the behavior of others, would be classified as prompting or probing . . . In deciding whether to spend our vacation in the mountains or at the seashore, for example, we may pore over travel magazines and vacation booklets, find out where our friends are going and what weather is predicted for each place, and so on. This material may, if we are unlucky, simply maintain the balance between the two courses of action, but it is more likely to lead to the prepotent emergence of one of them. (1954, p. 243)

Rather straightforward as Skinner wrote it then.

Describing his and colleague Daniel Kahneman's work on judgement (Staff, 1984), Tversky is further quoted as follows:

[We have] taken a critical look at a standard rational

analysis which has dominated much of the study of decision-making and judgement, particularly in economics. [Standard analysis has] taken the view that people can be described as rational decision-makers and evaluators of probability. These assumptions are very fundamental in economic theory and other aspects of behavioral sciences.

. . . rather than being rational maximizers of utility . . . people follow a limited number of what we call heuristics, or rules of thumb, for evaluating beliefs. Those rules are in many cases useful and general, but they lead to severe and systematic biases. What we've done is try to analyze those heuristics and the biases they lead to and the consequences of those errors to human conduct . . . (p. 3)

In cognitive language, Tversky reminds us that, traditionally, decision making and judgement have been viewed as if the putative decision making agent comes to some rational resolution of the relevant multitude of environmental variables that ensures an optimal outcome, but now he, together with colleague Kahneman, has determined that the decision making agent often uses rules of thumb instead. Presumably Tversky's rules of thumb are based on limited experience that simply lets one get by in the situation at hand but which may lead to error in another. Over fifteen years ago, in *Contingencies of Reinforcement* (1969), Skinner discussed rule-governed behavior and its relationship to behavior under natural contingencies. In developing his extended and systematic analysis of rule-governed behavior and its importance to science, Skinner was well aware at the outset of the concerns that constitute Tversky's "new" discovery. Skinner acknowledged the obvious. Since rules describe contingencies of reinforcement, the usefulness of rules depends on the accuracy and completeness of those descriptions. True; but hardly the important idea about rules—only an observed point of caution at the outset of a much more significant analysis that reviews in a major way the nature of verbal behavior and establishes the importance of its role in sharing the control of subsequent behavior with the natural environment. While the thrust of Skinner's analysis was directed at putting rule-governed behavior in its critical place, Skinner (1969), as an exercise in scientific care, made Tversky's point and related it to the contingencies governing rules of thumb:

... many proverbs and maxims are crude descriptions of social and nonsocial reinforcement The gain from any such discriminative stimulus depends upon the extent to which it correctly represents the contingencies which led to its construction The behavior of a poker player who evaluates his chances before making a given play merely resembles that of the player whose behavior has been shaped by a prolonged exposure to the game The results may be the same, but the controlling variables are different [The] behavior in response to [rules] is not the behavior generated by exposure to the contingencies themselves even when, on rare occasions, the two are similar. (pp. 123-124)

Skinner offers a few reasons for the usual failure of rules to produce the same quality of behavior exhibited under natural contingencies and concludes by reiterating that "rule-governed behavior is in any case never exactly like the behavior shaped by contingencies" (p. 150).

This extended example does not argue over whether Tversky or Skinner has the more effective repertoire to share in controlling responses to the phenomena of concern. Nor is this example pursued simply to suggest that further undue importance be attached to saying something before others say it. The example illustrates the operating style whereby the political majority appropriates control of the knowledge base, insures control over the professional recognitions, the acclaim, the enhanced opportunities, and even wealth (the MacArthur award included \$232,000 in cash), which all funnel to that political majority. The technically advanced minority remains a band of ignored or used persons whose ideas are rediscovered or adopted and then promoted as new intellectual merchandise.

An equally serious outcome for the few behaviorists, or the only behaviorist, in "study-of-the-psyche" departments results from the impact to their behavior, verbal or otherwise. Behavioral psychologists remaining within the traditional organizational framework of psychology are subjected to the powerful group contingencies of professional, social, political, and economic solidarity that prevail in the academy. Psychology faculty members are all colleagues. The behaviorist might be permitted polite, or even noisy, disagreement on technical mat-

ters, but academic propriety, if not self-restraint, prohibits criticizing the very basis of alternative training arrangements—their scientific legitimacy. The outcome of this sad state of affairs does not surprise. Both Morse and Bruns (1983) and Branch and Malagodi (1980) point out that in many cases no amount or kind of graduate training prevents the drift toward mentalism in a faculty member subjected to the continuous audience control of a cognitive community. As Branch and Malagodi (1980) put it, a behavioral faculty member, isolated among assorted cognitive psychologists, "eventually succumbs to the reinforcement and punishment practices of the immediate verbal community" (p. 36). Such persons usually rationalize their conceptual drifts—should students raise the question—by describing those drifts as "intellectual growth."

The necessary political compromises and accommodations forced upon behaviorists in a typical psychology department produce another serious outcome: collateral effects from the arrangements for training students that shape nonscientist professionals. Psychology students, from the outset, *observe* the political accommodations, the curricular compromises, and the undeserved authority accorded to persons whose phlogiston-type science does not deserve it. They *hear* the attempts of the isolated behavioral faculty members to justify on scientific grounds the personal political accommodations that obviously occur under nonscientific contingencies. And students learn. They learn that political accommodation takes precedence over good science. They learn that behavior analysis is weak around the edges and narrow in its coverage, and should give way to cognitive approaches in certain domains. They learn that feedback reinforces, that pigeons memorize when not hard-wired, and that behaviorists are nonhumanistic if not downright inhumane. They learn to believe what is expedient to believe. They become smooth-tongued accommodators. This slippage from contingencies of scientific work gives rise for concern; too many students neglect science to harbor ambitions of becoming admin-

istrators or entrepreneurs. Having gone on for a long time, this has produced "behavioral" psychologists who are deprived of the opportunity to lack the courage of their convictions by not getting very good convictions in the first place.¹

BEHAVIOR ANALYSIS AS A DISCIPLINE

In the second volume of his autobiography, Skinner (1979, p. 38) quotes from a passage he wrote in December 1928 during his first year of graduate studies at Harvard:

My present condition is excellent. I am working as hard as I have ever worked, but freely—with time and subject matter of my own choosing. I have almost gone over to physiology, which I find fascinating. But my fundamental interests lie in the field of Psychology, and I shall probably continue therein, even, if necessary, by making over the entire field to suit myself.

That remake has and has not happened. On the one hand, we seem to have a new discipline—the experimental analysis of behavior or, as usual, more briefly, "behavior analysis"—complete with its own philosophy, experimental data and methodology, and engineering technologies based on those data and that philosophy. On the other hand, behavior analysis continues to stay in psychology's nest, like an overgrown cowbird that is too big to be pushed out and too hesitant to fly out. What we presently call behavior analysis, however, occupies only a very small portion of the nest for there are many others and they continue to proliferate faster than it can grow. Nevertheless, behavior analysis is terribly resented. In between the pecks, one of the others occasionally chirps up and says, "It is dead. It is dead. All we have to do is bury it and get rid of the smell." (Note, for example, the sort of remarks made by Sigmund Koch in Wann, 1964).

Norman Guttman (1977) has pointed

out, appearances and anxieties notwithstanding, that behaviorism has always been out of the mainstream of psychology:

B. F. Skinner, happily, is still with us, and his later years have been very productive and influential, but if we were to look through the *Journal of Experimental Psychology* for indications of his prominence, we would find that, in none of the volumes sampled, is he referenced more than a couple of times per year; his channels to fame by-passed the main stream of the American psychological establishment. Skinner and his followers, as we shall relate, made their own media and have formed their own establishment. (p. 322)

Guttman noted that Skinner established his credentials through the impact of his ideas on applied areas—such as education—outside the field of psychology and that his philosophy has gained wider acceptance in some of those areas than within established psychology. Skinner's subsequent emphasis and advocacy of radical behaviorism has moved the scientific study of behavior even further away from psychology.

In a sense, it is almost an accident that psychology can claim behavior analysis as one of its many tributaries. Most of Skinner's early work and his first publications were in the field of biology. Though a student in the psychology department he held his research appointment in the department of physiology working under Crozier, who "was fascinated by any demonstration of lawfulness in animal behavior" (Skinner, 1979, p. 45) and who defined physiology as "orderly processes in the organism as a whole" (p. 60). In 1929, Skinner wrote to his parents

You see the physiology of the nervous system is practically psychology and the facilities of the Department of Physiology are better It would mean not only that a Ph.D. from Physiology would be a better thing, but that there would be a good chance to line up with a local laboratory under this new endowment fund and get a good position, with nothing to do but my own research. (Skinner, 1979, pp. 25–26)

It was in one sense a matter of convenience that he stayed in the department of psychology.

I was confirmed in my choice of psychology as a

¹ For an extended discussion (1) of the implications in the compromises of belief systems, see L. E. Fraley, 1984, and (2) of the training students receive, see J. L. Michael, 1980.

profession not so much by what I was learning as by the machine shop in Emerson Hall. (p. 31)

The shop became the center of my activity. (p. 32)

By and large he was left alone, and took the fewest of courses—in which he merely got passing grades. But remaining in such an isolated position facilitated developing his strong conviction that behavior was a subject matter to be analyzed in its own right. It was not to be understood either as an outcome, in the organism, of actual physiological states or hypothesized psychological states. Behavior resulted from the organism's interaction with the environment, and both organism and environment were necessary substrates for the behavior to occur. Each substrate had to be examined, and manipulated, for what each contributed to the subject matter called "behavior."

Such an emphasis came much closer to that of ethology than to the core of mainstream psychology. The difference was that Skinner was interested in variables that operated over the lifetime of the individual organism to modify its activity. Ethologists were interested in variables that operated over the lifetime of a species to modify activities shared by all members of that species. For both Skinner and the ethologists the environment was absolutely necessary: to occasion or to cue an activity, however complex, and thereafter shape it. Neither Skinner's analysis nor that of the ethologists required psychological states to explain the behavior observed, though most ethologists inevitably ended up psychologizing. Ethologists, however, did not accept Skinner's analysis any more than the psychologists did. While the latter understood him and rejected him, the former misunderstood him and rejected him. Ethologists never understood that Skinner accepted that membership in a species might dictate a wide range of behavior, entering even into the most complex of repertoires.

Behavior analysis, or whatever Skinner's brand of behaviorism is eventually called, may belong in a discipline niche between ethology and psychology, but the most logical position is that it encom-

passes both. Michael (1985) strongly implies that Skinner's scientific framework and philosophic position—radical behaviorism—incorporates ethology. That appears to be an accurate assessment. Skinner argues the necessary concern behaviorists should have with phylogenetic variables, and insists that these should be incorporated in any thorough analysis and explanation of behavior.² It just happens that Skinner's work focused on the important though heretofore badly analyzed ontogenic domain. A faulty analysis still characterizes present-day psychology: Most psychologists, and their fellow travelers in the other behavioral disciplines, still assert that will, desire, and intention govern most of human behavior. As radical behaviorists, those are not our explanations, though what we say attempts to explain, and often does explain, the actions labeled by those terms. The experimental analysis of behavior and radical behaviorism preempt psychology and now cover the same subject matter, including culturally mediated and instructionally governed actions. As Michael (1985, p. 19) puts it, "Behavior analysis . . . is just scientific method applied to behavior in all its manifestations It is, in short, the science and technology of behavior."

PROBLEMS OF AN EMERGENT DISCIPLINE

Overview

We should acknowledge professionally what we believe philosophically, do scientifically, and apply technically. We differ from other professionals in all the crit-

² See as examples, though not the only ones: "The phylogeny and ontogeny of behavior" and notes following that chapter in Skinner, 1969; chapter 3 in *About Behaviorism*, 1974, but especially "Intermingling of contingencies of survival and reinforcement," pages 40 to 44; Skinner's reactions to reviewers' comments following those papers dealing with phylogenetic issues in *The Behavioral and Brain Sciences* issue, December 1984, co-edited by Catania; Vaughan and Michael's (1982) discussion of automatic reinforcement, particularly pages 224–225; and the comment by Skinner (1977, p. 1011) in his answer to Herrnstein.

ical domains that define a discipline—except one. We as yet do not have an academic home—educational units that will produce more radical behaviorists. Radical behaviorists are so scattered among the few campuses on which they do reside that they never form the critical mass sufficient to overcome the bureaucratic and political difficulties to start an independent academic unit. The one or two people in our field with sufficient stature to be given the resources to start an independent department retired from organizational work after doing much to initiate the growth of the discipline. Unless we provide for faster growth than now apparent—and explicit independence as a discipline is requisite to that growth—the integral culture that is now radical behaviorism will simply fade.

Instead of, as often said, behaviorizing the culture, the culture will cognitivize us. We see the effect in our journals, our practices, and in our conference. Already, some of us in print, and many in private, complain about the number of articles with terms and methodologies expressive of traditional cognitive analysis creeping into the journals started to foster the experimental analysis of behavior and its applied technology. Creeping cognitivism, Jack Michael calls it. Some graduate students whine, *“People don’t like us. Why do we have to use our technical terms even for everyday stuff? They sound so odd and sterile; yes, even inhumane. Can’t we just sneak in the right techniques, using every day language and not worry about the language, or the science and what the science behind those techniques implies? That way, more people will like us and we’ll make more progress.”*

Certainly, once no longer students, that is what many practitioners do. So we find increasing use of techniques derived from radical behavioral science in special education, in business and industry, in family counseling, and so on, and used because they do work and they do help. But no recognition of the science—especially of its philosophically uncompromising deterministic approach to human affairs—from whence those techniques came. And so radical behaviorists are seen

as a sort of lunatic fringe muttering over their Skinner books, while the grants, the acclaim, the support, go to those technicians who dress up what they do in the conventional wisdom, backstaging the science responsible for their success. Doing good, some call it. Every year we have people show up at our association conference to tell us how wrong we are, and we listen respectfully wondering why, with so many other conferences for their complaints and criticisms, they choose this one to address. No great harm done except to those who do not know any better. Debate sharpens the wits. But do we have to travel to hear the same old stuff we hear at home? The excuse goes, “we’re influencing the cognitivists who don’t mind paying the conference fee.” For those who come to resolve differences, and who know the differences, that is true; but for the rest their cost is the price of admission and our gain a bit of silver. Staying alive, the accommodators assert. This may all sound like wholesale rejection of other points of view. True; it is. And it should be. It is a matter of controls over our verbal behavior and of the kind of verbal community we arrange to provide those controls.

In suggesting solutions to the problems of becoming an independent discipline, developing an academic home to reproduce our scientific culture is the larger problem. The middle-sized problem is the organization we should have to foster our radical behaviorism. (For example, the tenor of the organization would change considerably if it were “of behavior analysts” instead of “for behavior analysis.”) The smaller problem is what we call ourselves. The professional community of our discipline must collectively discuss and resolve these three issues. This article represents another effort in this growing preoccupation with the evolution of our discipline, and in this last section we address the “smaller problem,” the matter of giving it a proper name, guided as we do so by the assumption that the more important discussions to follow on future occasions will be facilitated by the availability of accurate and tested terms.

A Professional Name of Our Own

The name gains importance from denoting directly that to which we willingly commit ourselves, and consequently it accelerates the process of shared professional identity. A name connotes a point of view, and sometimes represents it strikingly—astrologers and astronomers both study the same subject matter. To gain the status necessary for the academic independence of the discipline, as exemplified by new academic departments, we must call our field of study by a name descriptive of behavioral phenomena but which is sufficiently delimiting to justify excluding appeals to cognitive explanations and the people who resort to them. But our history of struggling with names has been troubled.

A chronicle of that history has recently been provided by Epstein (1984) as a prelude to his argument that we adopt a newly synthesized term, “praxics,” for our behavioral discipline. Epstein asserts that the new term broadly covers most, if not all, of what must occur within a scientific study of behavior plus the development of behavioral technologies. As a new term, its advocates could define it and interpret it so as to preclude cognitive corruptions. But unfortunately, that new term has, in our view, already been mismanaged in such a fatal way as to destroy its potential utility for our purposes. Its problem is not that it is new and strange. With no reinforcing history behind it, and with everybody’s common punishing experiences in coping with unfamiliar terms, nobody likes *any* new term very much. “Praxics” is defined as the disciplinary locus of persons who approach behavioral problems *experimentally* and presumably without the shortcuts afforded by cognitive hypothetical constructs—a definition that would raise no eyebrows in any garden variety behaviorist; just a bit of puzzlement over why a label for only one aspect of scientific endeavor. Its problem is that Epstein rejects philosophy as being important to good science. But the verbal repertoires that scientists bring to their data predispose them to interpret those data in particular ways.

As Darwin put it, “How odd it is that anyone should not see that observation must be for or against some view if it is to be of any service” (Hull, 1973, p. 9). The theory of evolution was as much a philosophical solution as it was a scientific one, and so was Copernicus’s solar hypothesis since Ptolemy’s epicycles accounted for the known facts. Closer to home, witness the variety of ways experimenters interpret the same data in the experimental analysis of behavior, from whether any distinction exists between operant and respondent conditioning to assertions about memories and signaling repertoires.³

Epstein did not single out radical behaviorism and explicitly reject it. He simply denounced philosophy in general as relatively useless to an experimental science, and then failed to except and reinstate radical behaviorism. Instead, he allowed it to languish undifferentiated from the whole lot of philosophies, so deliberately and completely eschewed that “praxics,” the proposed new label, entertains no distinctions among an absence of philosophy, bad philosophy, and the kind of critically important philosophy that made possible the emergence of radical behaviorists from methodological ones.

Praxics welcomes philosophical dualists—the mystical, the religious, the mentalists—welcomes them, we are told, because “no laboratory science should be constrained by a philosophy” (Epstein, 1984, p. 111). This approach not only runs counter to Branch and Malagodi’s (1980) admonition in discussing the importance of controls over the scientist’s verbal behavior, that it is not all right “to talk funny as long as you do the right things” (p. 33), but also duplicates the current situation in psychology, a curious duplication since a good portion of Epstein’s article recounts the struggles of behaviorists to change psychology, or leave

³ For an extended discussion on multiple controls over the scientist’s behavior, especially audience control in relation to the subjective-objective distinction, see Vargas (1982).

it, due to the philosophical dualism anchored by psychologists' enchantment with "mind" and other "essences" more than slightly redolent of soul. Such a call for anyone willing to sign up sounds more like a movement for a political party than a scientific discipline, as if biologists in their enthusiasm to win friends and influence legislatures should invite creationists to join them in one big happy family, since their togetherness should not be constrained by the different interpretations of the same facts they encounter in the laboratory and in the field.

What distinguishes behaviorists from cognitivists? It clearly is not in what they observed, for both observe what people do. It is not in adherence to scientific technique, for many cognitivists pride themselves on their rigorous scientific approach, and other professionals in other scientific disciplines applaud them; many cognitive articles appear in *Science*, more it seems than behavioristic ones. It certainly is not in applied work; cognitivists are more than happy to do good with behavioral techniques that work. And it most certainly is not in laboratory work: Since the days of brass-instrument psychology, cognitivists have carried out most laboratory based studies. Such laboratory work is not restricted to their choice of subject and technique; lately, laboratory studies with single-subject methodology on topics traditional to the experimental analysis of behavior have appeared in the *Journal of the Experimental Analysis of Behavior* interpreting at least in part, with cognitive terms, the data obtained.

Michael (1980), sometime back, in his Association for Behavior Analysis presidential address, and recently Hineline (1984) and Leigland (1984) describe the distinction in various ways, but all point to the philosophy called "radical behaviorism." With respect to the importance of this philosophy, Michael makes the case that in educating new behaviorists, it is as important for them to learn the philosophy of the experimental analysis of behavior as its laboratory techniques and that the shift of emphasis from radical behavioral science to professional

repertoires with which to do good results in new professionals not acquiring the science or the philosophy of that science responsible for the engineering technology. Hineline (1984) points out that the interpretation of behavioral phenomena in cognitive terms is incompatible with a behavioristic one, and probably always will be, for the frameworks of interpretation are fundamentally opposed. Leigland (1984) responds to Skinner's question whether the experimental analysis of behavior can rescue psychology by questioning in turn whether such a rescue can ensue from laboratory-based, experimental work. As Leigland puts it, "If the experimental analysis of behavior is to rescue psychology, it will only be through the guidance of radical behaviorism" (p. 74). Along with many others, however, we agree that psychology is a lost cause, and that our efforts should go to maintaining and improving the shared scientific culture we have achieved, before we are forced to do some rescue work among ourselves. That shared culture, called radical behaviorism, denotes, with respect to our verbal behavior, the controls that determine how we will react to a given body of facts, and even what will be admitted as facts.

A discipline for the study of behavior accommodates many facets: appropriate philosophy, experimental work, and engineering applications. Because psychology as a discipline already preempts, and continues to emphasize, the study of cognition, mind, and psyche, and because people collect there who concern themselves with such qualities, the name selected for the discipline of behavior need only mean the study of behavior and its related events, and need not carry the burden of explicitly excluding cognitive scholars and subject matters. The cognitive scholars already reside comfortably in their clearly defined home, and thus call themselves "psychologists." For what we believe, and do, "behaviorist" sounds like a good term, but few seem willing to say it. But what appears wrong with "behaviorist" that a smidgen of social courage would not overcome? It goes back nearly a century, to when Watson,

in his straightforward manner, coined it, and the best in our science have honored it by calling themselves "behaviorists." And many still do; one well known autobiography is titled, "*The Shaping of a Behaviorist*." People who study physics call themselves "physicists"; those who study chemistry call themselves "chemists"; those who study biology call themselves "biologists." It seems reasonable that those who study behavior should call themselves "behaviorists."

Our discipline needs a term descriptive of our science in its broad sense, and that term is not "psychology." Too many people hold other meanings for that name. We would never win the battle over what it denotes. The custody fight is lost already. And in continuing to struggle for it, we could easily lose our identity. Whatever term is chosen, "behavior" should be its stem, for our efforts focus there, not in the putative underlying psyche.

There are a number of discipline names possible with the stems "behave" or "behavior." We must attach one of the three possible suffixes that indicate a discipline or domain of scientific study. These are: -ics, -ry, and -logy. Thus, using "behavior" as the stem, we come up with "behaviorology" as in bacteriology, mineralogy, or meteorology. No doubt when those who study weather, minerals, and bacteria first named their fields those names sounded strange, and probably even cacophonous, to their ears and certainly to those of others. We say "bacteriology" quite naturally now. If convention so dictates, we will just as easily say "behaviorology." Or taking advantage of "behave" as the stem, we could coin "behavology" as in the quite familiar "psychology" or "theology" or "biology." "Behavology" also sounds strange but if we use new terms they stick around, and from proctology to urology professionals get accustomed to the sounds of the words their disciplines demand, however those terms may strike others. Equivalent combinations could be formed with "-ry" and "-ics." In any case, a variety of names could be minted, all perfectly respectable according to the rules

of usage, which once adopted will, through the habit of usage, sound as natural as our own names. We all know this. It is simply a matter of agreement.

We thus would name our discipline and that would name the academic departments in which training for that discipline occurred. Our basic scientific discipline would be to the many applied behavior fields—education, counseling, clinical therapy, personnel management, and so on—what physics is to the various branches of engineering, namely, the basic conceptual and scientific framework from which those fields derive their strength. Furthermore, physics has its theoretical and experimental aspects just as "behaviorology" does. Both theoretical and experimental experts would work in the same department. And both would work together to produce the behaviorists so badly needed.

CONCLUSION

We address a particular question: How best can we move forward the science of the analysis of behavior? That question receives a number of different answers. Witness the various efforts devoted to the conceptual, experimental, and engineering issues of our discipline. The question also raises professional issues. At present one stands above the rest: Will our discipline prosper most as a subsidiary branch of psychology or as a full-fledged independent discipline?

Any answer to the latter question must take into account the social nature of the scientific enterprise. Science is a set of practices, and like any social endeavor captive to political and economic forces. One of these is the tendency for those who control a profession to continue to do so for reasons that have little to do with the science. Turner (1985), in a review of two books on the rise of the Chicago school of sociology, put it this way,

For virtually all organized intellectual activity involves competition among universities and their faculties, who often gain hegemony by producing paradigms that dominate the conduct of inquiry, at least for a time, and who exercise control over the flow of not only intellectual but also financial resources (p. 851).

The dominant paradigm in psychology is cognitive, enthusiastically supported by the lay culture through government bureaus and private agencies with money to dispense. Within departments that study the psyche, the cognitive paradigm not only seems scientifically proper but downright necessary to maintain social support for the discipline of psychology. An occasional token behaviorist may be tolerated to maintain the fiction that in science a thousand theories may bloom, all happily cultivated without interference. But if these behaviorists become too effective (with learning centers that actually produce learning) or too many (two or three seems to be the number here), countercontrol measures soon occur. The learning centers close; promotion and tenure possibilities evaporate. It is little wonder that the operating dictum for many behaviorists, when faced with the possibility of taking a strong stand for their science, such as independent professional status, is that of keeping a low profile; a position that has nothing to do with their integrity as scientists but much to do with their shaping as politicians. We are not free in this matter any more than in any other. Our behavior, as practicing behaviorists, reflects current social forces, and discussions of the status and future of the field are not only important for resolving what might be best both for the near and far future but also to give weaker variables their play.

The question immediately arises: If we are not strong enough to dominate and eventually change the discipline of psychology, how can we gain the strength to initiate, maintain, and make prosper our own discipline of behaviorology—or whatever we may call it? This, of course, is a very practical question, one of how we can program the change from perspective to profession. If we look around, we see that we have accomplished quite a bit already: journals, a professional organization, thriving regional organizations, the initial machinery to credential behavior analytic expertise, a name (behavior analysis) for our scientific and engineering efforts, and now a current concern over what we should name our

profession so that it reflects our philosophical perspective with respect to what we study and how we explain it. We lack, and it hurts us sorely, academic homes with which we can reproduce ourselves, for as earlier discussed, and stressed by others as well, there is little chance that we can adequately train people to study behavior in departments geared to produce people to study the psyche. Without these repertoires, behavior analytic science simply disappears. Former program steps, though successful, have been taken almost by accident; we increase the probability of success by planning for them.

But there is an earlier and more important question prior to those of program issues: Do we wish to become an independent discipline? In a slow, hesitating, and clumsy way, we have been answering "Yes," for an independent discipline announces what we are, and what we are profoundly differs from any other discipline. But such effort concerns not only identity. Independence is simply the name for the sorts of controls we prefer and under which we would prosper.

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